SCIENCE

FRIDAY, FEBRUARY 3, 1888.

THE COMMISSIONERS OF PUBLIC SCHOOLS of Baltimore, Md., have taken the initiative in the introduction into the public schools of that city of a series of reforms which, if adopted, will be of great benefit to the pupils. This action is the more noteworthy from having originated in the school board, the proposition coming from its committee on health, and not being forced upon it by the board of health or public opinion. It is greatly to be hoped that the mayor and common council of the city will give the necessary power and money to carry these resolutions into effect. After a preamble to the effect that sanitarians and teachers have proven that children attending school are frequently subjected to influences prejudicial to health, which often leave their effects upon the constitution for life, and that it has been demonstrated, that, by expert sanitary supervision of schoolhouses and of the pupils themselves, many of these injurious influences can be mitigated and removed, the resolutions are, that the mayor and city council be requested to authorize the commissioners of public schools to appoint an officer, who shall be a physician and expert in sanitary science, to be known as the sanitary superintendent of public schools, whose duty shall be, 1st, to carefully examine all plans submitted for the construction of new schoolhouses, and suggest such modifications as may be necessary from a sanitary point of view; 2d, to advise with the commissioners with reference to necessary alterations in schoolbuildings to improve their hygienic condition; 3d, to examine all text-books before adoption, in order that type, printing, or paper injurious to the eyesight of pupils may be avoided in selecting such books; 4th, to satisfy himself, by personal examination if necessary, that all pupils admitted to the schools have been properly vaccinated or are otherwise protected against small-pox; 5th, to take such other measures, in conjunction with the health commissioner of the city, as may be necessary to prevent the spread of contagious diseases in, or through the medium of, the public schools; 6th, to examine annually the eyesight of all children attending the public schools, and keep an accurate record of such examinations; 7th, to report annually, or as often as may be required by the commissioners, upon the sanitary condition of the schools, and of the pupils attending them, and to advise the commissioners upon sanitary questions connected with schools whenever required; 8th, to give instruction, by lectures or otherwise, to the teachers in the schools upon the elementary principles of school hygiene.

ASSISTANT CHARLES A. SCHOTT, assistant in charge of the computing division of the Coast and Geodetic Survey, has addressed a letter to the superintendent of that bureau which states that the discovery of records of the magnetic declination, A.D. 1714, off the coast of Mexico, by Assistant G. Davidson, and transmitted by him to this office, Dec. 7, 1887, proved to be a matter of much importance by greatly increasing our knowledge of the secular variation of the declination. By means of these observations we are able to improve materially the expressions for San Blas and Magdalena Bay, to add the new station Cape San Lucas, and to make their influence felt as far north as San Diego and Santa Barbara. It is the range which is greatly improved; besides, the epoch of maximum declination is shifted in the right direction. Apart from the fact that a region of west declination is here for the first time observationally indicated on the Pacific coast, the power of the newly recovered declinations is due to the circumstance, that, as far as known, they cover a time when the needle was in or near a phase the opposite of the present one. For want of early observations, these previously collected for San Diego and Santa Barbara, Cal., were extremely difficult to handle; and, while it was not an easy matter to establish new expressions for these stations, their correctness, or rather applicability over the whole period of time the observations cover, is quite re-assuring. He points out the desirability of new observations (either using funds yet available before July next, or providing funds to be used after that date) at San Diego, Santa Barbara, and Monterey, and states that these stations have received no attention for seven years. These observations are demanded to give greater precision to the computed variations on our charts.

ASA GRAY.

PROF. ASA GRAY died at his home in the Botanic Gardens, Cambridge, Mass., on Monday evening, Jan. 30. He had been unconscious since last Thursday, and helpless for more than a month.

Dr. Gray was born at Paris, N.Y., Nov. 18, 1810. He took the degree of M.D. at Fairfield Medical School, in 1831, but never practised medicine. After a short time spent in teaching some branches of natural history in a private school in Utica, he was induced, through correspondence with Dr. Torrey of New York, a professor of chemistry but more widely known as a botanist, to accept, in 1833, a position in his laboratory, and a little later that of curator in the Lyceum of Natural History. By Dr. Torrey's side, he began a career of ceaseless botanical activity.

His botanical publications were begun with a description of certain sedges and newly discovered plants of north-western New York. In 1835 appeared 'North-American Grasses and Sedges,' and in the following year 'The Elements of Botany.' This last was more than a mere compilation of the materials available at the time, and gave a good account of what was known of the principles of morphology, histology, vegetable physiology, and of the department in which Mr. Gray was more interested, botanical classification. Although the young writer ventured to differ from the authorities of the day, he was happy in after years in finding that these expressions of his youth needed but little change. His 'Botanical Text-Book' was published in 1842; and with this we may refer to the educational books written by him, which comprise a long list: 'How Plants Grow' (1858), 'How Plants Behave' (1875), 'The Lessons' (1857), a new edition of 'The Elements' (1887), and the 'Text-Book,' issued during the past year, which is a revised edition of 'The Lessons.' Besides these, we may mention 'The Manual of the Botany of the Northern United States' (1848), of which there were five editions, also 'Field, Forest, and Garden Botany' (1868). His 'Manual' is probably the best known, as it must have been in the hands of every American botanist since the time of its publication. The 'Genera of North America' he began in 1848, but of this but two volumes have been published, which, even in their unfinished condition, have been of great use to botanical teachers. The great work of his life is the great 'Synoptical Flora,' which had its beginning in Torrey and Gray's 'Flora' forty years ago. As far as published, it consists of a volume of nine hundred and seventy-four pages on the gamopetalous orders, but there are other portions which have been published in the Proceedings of the American Academy. It would be impossible to enumerate the numerous memoirs and papers which have come from his pen, many of which have been tributary to the 'Flora.' Dr. Gray regarded as his most important minor work 'The Relations of the Japanese Flora to those of North America,' published in 1859. This was based on the study of plants collected by Wright, and he believed this paper gave him his reputation to a large extent in Europe.

In 1834, or possibly later, Dr. Gray received an appointment as botanist of the Wilkes expedition, which was expected to start for the South Sea Islands; but delay after delay, and a change in the plans of the expedition, caused him to resign, and about this time he received an appointment to the chair of botany in the University of Michigan, then just established. He asked for a year's absence in Europe, which was granted him, during which year he not only made valuable purchases and collections for the library of the new university, but gained the personal acquaintance of the leading European botanists. He made himself also familiar with the type specimens in the older herbaria, and came back fully equipped for the work of his life, the examination of the North American flora, the first volume of his 'Flora' being completed in 1840. He never occupied the chair at Michigan, but in 1842 accepted a professorship of natural history in Harvard. The early years of his life in Cambridge were naturally occupied with routine teaching, with appliances which would be regarded as utterly inadequate at the present time; but it was a small beginning, which has led to the better-equipped Botanic Garden and to the establishment of an herbarium. He continued his work as an instructor till 1872; but during this time he found opportunities for carrying on his work on the 'North American Flora,' for the preparation of his educational treatises, and for increasing the popular interest in science. In addition to this, he devoted much time to the American Academy, in which he always took the greatest interest.

To the public not merely interested in the science of botany, especially to the religious public, Dr. Gray is well known by his writings on the relations of science and religion, and upon the Darwinian theory. Darwin, in his letters recently published, refers to Gray as one of the three or four whose judgment on his theory was of more value to him than that of the world besides, including with Gray, Hooker, Lyell, and Huxley. Darwin had been in correspondence with Dr. Gray for years before the publication of his great book, and had been gathering from him botanical facts bearing upon his hypothesis; and from the time of the appearance of that volume Dr. Gray was one of the earnest advocates of the theory as a fair working hypothesis. Many residents of Boston and vicinity will recall the earnest discussion before the American Academy, in the years 1860 and 1861, between Dr. Gray and Professor Agassiz on this great question.

Dr. Gray was crowned with diplomas and honors from all the principal universities of Europe, and during the past summer, while travelling in England, received degrees from the Universities of Oxford, Cambridge, and Edinburgh. He leaves no children, but a widow, the daughter of the late eminent lawyer Charles G. Loring of Boston; and a host of friends in Cambridge and throughout the country will feel that his death has extinguished a bright and cheering light in the world of thought, and has removed a most cherished and valued friend and companion.

HEALTH MATTERS.

Sex and Consumption.

DR. THOMAS J. MAYS of Philadelphia has contributed to the *Medical News* a very valuable paper on female dress as a determining factor in pulmonary consumption. He says that it is currently believed that more females than males fall victims to this disease. Both Laennec and Louis held this view, at least so far as France was concerned; and Ancell, one of the most prolific and exhaustive writers on the natural history of tuberculosis, concurs entirely in this opinion. Sir James Clark's statistics, which were collected from thirteen different localities in Europe and America, showed in the aggregate more deaths from phthisis among females than among males. Dr. A. James, in an interesting article on sex in connection with phthisis, lately published in *The Edinburgh Medical Journal* (March, 1886), arrives at the same conclusion. It must be admitted, too, that, if the question of sex in relation to pulmonary consumption be viewed from an *à priori* standpoint, there are sufficient reasons for believing that the female is more prone to the disease than the male, because she is generally considered to be the weaker, and because she is more exposed to the causes which are known to give rise to it. She is confined within-doors, where she naturally spends the greater portion of her life, and is, of course, subjected to the influences of impure air and bad ventilation. She leads a sedentary life, is deprived of sunlight, exercise, and undergoes the enervating processes of gestation and lactation, while, on the other hand, the male is, as a general rule, less or not at all exposed to most of such unhealthful conditions; and it is only when he is subjected to some of them, as, for instance, to impure air, sendentary occupations, etc., that he becomes notoriously liable to pulmonary consumption.

Dr. Mays has collected statistics for many of the American cities, and also for other countries, and finds, that, so far as they go, they establish the fact beyond a doubt, that in civilized life the male sex is more liable to pulmonary consumption than the female. He gives the following statistics:—

STATISTICS OF SEX IN PULMONARY CONSUMPTION.

Locality.	Male.	Female.	Remarks.
Chicago [1]	1 : 635	1 : 793	Average for 6 years, 1869, 1881–85.
New York City, 1870	1 : 233	1 : 318	
Massachusetts, 1880	2.86%	3.28%	Per 1,000 males and females respectively.
Boston, 1883–84	1 : 248	1 : 251	
Rhode Island, 1884 and 1885	1 : 380	1 : 351	
Philadelphia, 1884 and 1885	1 : 303	1 : 310	
Nashville, 1877 and 1878	1 : 263	1 : 286	Both white and colored males and females.
" " " "	1 : 443	1 : 422	White males and females only.
" " " "	1 : 142	1 : 190	Colored males and females only.
San Francisco, 1875–80	1 : 313	1 : 418	Average of 5 years.
Sacramento	1 : 340	1 : 435	Average of years 1874 and 1879.
Cincinnati, 1883	1 : 325	1 : 423	
Baltimore, 1885	1 : 342	1 : 381	
Scotland, 1871–80	1 : 423	1 : 387	
England, 1872–81	1 : 467	1 : 502	
London, 1843–46, decedents from consumption	53%	47%	See 'Ancell,' p. 396.
Basel and Zurich, 1877–84, decedents from consumption	54.7%	45.3%	See Dr. Schnyder in *Correspondenz Blatt für Schweizer Aerzte*, Nos. 10, 11, 12, 1886.
Cantons of Wallis, Waadt, Freiburg, Lucerne, from 1877 to 1884, decedents from consumption	52.5%	47.5%	*Ibid.*
Cities of Prussia, 1875-79	1 : 236	1 : 318	
County districts of Prussia, 1875–79	1 : 314	1 : 369	
Leading cities of New Jersey, 1884, deaths from consumption in nine	94	84	

Hospital and Private Practice.	Male.	Female.	Remarks.
Dr. Flint, Sr., 669 cases	505	164	See 'Flint on Phthisis,' p. 50.
Dr. Williams, 1,000 cases	625	375	See 'Williams on Consumption.'
First Brompton Hospital report, 1848	61	39	
Dr. Pollock's practice	60.75%	39.25%	
254 patients of Dr. Schnyder's, coming from cities	165	89	See Dr. Schnyder, *Cor. Blatt für Schweizer Aerzte*, Nos. 10, 11, and 12, 1886.
914 patients of Dr. Schnyder's, from the country	537	377	*Ibid.*
500 of Dr. Brehmer's cases	319	181	See Brehmer, 'Die Aetiologie der chronischen Lungenschwindsucht.'
88 cases reported by Dr. Churchill of Paris	59	29	
67 cases reported by Dr. Thorowgood	34	33	
Cases in Royal Infirmary, Edinburgh, 1833, 1834, and 1835	365	217	See Reynold's 'System of Medicine,' vol. iii. p. 546.
Consumptives in three Parisian hospitals, proportioned to the whole number of inmates	1 : 35	1 : 21	See 'Ancell,' p. 397.
550 deaths in St. George's Hospital in ten years	388	162	*Ibid*, p. 763.
Chest Department of Phila. Polyclinic since Jan. 1885	113	88	
Brompton Hospital for Consumption, from 1842 to 1848	2,682	1,597	

[1] These figures indicate a lower death-rate for Chicago than actually exists, because we are not able to obtain the male and female population of this city separately: hence our estimate is based on the male and female population of Cook County, in which it is located, and for comparative purposes answers very well.

Having seen, then, that in the aggregate more civilized males than females die of pulmonary consumption; that the costal respiration of the civilized female is developed through the constricting influence of dress around the abdomen; that the lungs possess an excessive breathing surface which by sedentary occupations, etc., can be, and is frequently, reduced in a great degree; that the baneful results of such a reduction, consisting of hyperæmia, etc., fall with the greatest force on the apex of the lung; that all those who suffer from consumption also show a decided tendency to immobility of the upper part of the chest, — are we not, therefore, justified in believing that a defective costal respiration and the beginning of pulmonary consumption stand in relation to each other as cause and effect? And, going one step further, is it not clear that the civilized female owes her greater immunity from this disease in a great measure to the fact that she possesses a more highly developed costal expansion? If these relations exist, it is quite obvious that her manner of dress is a direct factor in bringing about this result. She has, by force of necessity, been led to clothe herself after a method which demands a restriction of the abdominal and diaphragmatic movements, and which cultivates a greater development of the costal portion of the breathing-organs, and thereby she unconsciously protects herself to a greater degree against this disease; while the male, on the other hand, dresses himself after a fashion which secures perfect fredom of motion to the diaphragm and to the abdominal muscles, but which also attracts and tends to confine the respiratory function to the lower portion of the chest. Moreover, it must not be forgotten that the same fashion also demands that his clothing should be suspended from the shoulders, which of itself restricts the movements of the upper portion of the chest; making it evident, therefore, that his clothing renders him, both indirectly and directly, more liable to the disease under consideration. To this conclusion, and to no other, do our facts and reasoning lead.

The evidence which has thus far been gathered from statistical, experimental, and inductive grounds, all tends to demonstrate that impairment of the respiratory movements of the upper portion of the lungs is one of the principal direct causes of pulmonary consumption. Indeed, all the proof goes to show that in many conditions of life, especially in many of those to which the male sex is exposed, the apices of the lungs become superfluous parts of the body, and on this account possess a strong tendency to that premature waste which is characteristic of all organs when they fall into a state of inactivity. The practical solution of the problem of the prevention of pulmonary consumption, as well as of the cure in many cases, therefore consists in the adoption of measures which tend to increase the chest capacity, and which maintain the general and local health of the individual.

The treatment, so far as prevention is concerned, resolves itself into a proper exercise of the chest muscles, into systematic breathing, and into the rational employment of compressed and rarefied air. First, then, as to a proper training of the chest muscles. This is accomplished by raising the shoulders, and by swinging the arms backward, forward, and upward, either with or without dumb-bells, or by exercising on parallel bars, care being taken that a full inspiration is taken every time that the arms are thrown backward and upward, or the body forward, and that a complete expiration occurs when the arms are brought together in front, or when the body is thrown backward. These movements should be performed regularly, and from sixteen to twenty times in a minute. There are a number of appliances in the market which are worked by means of ropes, weights, and pulleys, and which are admirably adapted for the enhancement of the above-described movements. They are very simple, and can be attached to the wall of the nursery or of the sleeping-room, and not only afford a healthful exercise, but a pleasant amusement for both children and adults.

BOOK-REVIEWS.

Handbook of Republican Institutions in the United States of America. By DUGALD J. BANNATYNE. New York, Scribner & Welford. 16°.

THIS would have been a remarkable book even for an experienced public official to have written, and it is doubly so, coming from a foreigner. It is the most systematic, the most complete, and the most accurate handbook of our institutions that has ever come to our notice. The author is a Scotch attorney, resident for twenty-two years past in Canada and the United States. In his preface he gives the reason for writing the book. "I have frequently heard it said," he writes, "that an immigrant into either of these countries, who brings some capital with him, is not likely to permanently succeed until he has lost all he brought with him and has started afresh." The author's personal experience corroborates this saying, and he attributes its truth to the fact that immigrants are ignorant of the country, the people, the customs, the government, to which they have come. To aid such in removing their ignorance, the book before us was written. But its existence can be and should be defended on far broader grounds. It is a mine of information for the American citizen himself, or at least it would be if it had an index. No table of contents, however full, can take the place of a good index.

The work consists of an introductory chapter and two parts. In the introduction the author gives a cursory view of our political life, its conditions, and its most recent workings. Rather too many statements rely upon the somewhat vivid and always vehement imaginations of the New York *World* for their foundation; but this is a minor matter, and may be overlooked. Mr. Ballantyne betrays his foreign extraction by criticising the equal representation of States in the United States Senate. The populations and areas which he cites as evidences of inequality of representation have absolutely no bearing upon the question whatever; for it is States as such, and not populations or areas, that are represented in the Senate. In these days, when so much ignorant criticism and unintelligent abuse are directed at public officials, it is pleasant to find that a disinterested and presumably non-partisan foreigner can write as follows: "The reader's attention should be attracted by the manner in which the whole population is, through Congress, kept thoroughly posted as to the several executive departments, and the whole United States and state, county, town, village, and city machinery. . . . The writer has on several occasions tested the merits of the federal, state, county, and other public officers, and has always had prompt response and courteous treatment. There is no unnecessary red-tapism or flummery, and every respectful application, whatever the form, receives attention" (p. 51).

Part first contains the great national documents, — the Constitution, Articles of Confederation, Declaration of Independence, and Washington's Farewell Address (which in some places the types make 'Farewell Letter'). Then follow careful, painstaking, and accurate descriptions of Congress, the Presidency, the Executive Departments, and their numerous bureaus and divisions. Every branch of the national administration is touched upon. The Territories are described, and the Enabling Act of Colorado given, to show by what process a Territory becomes a State.

Part second treats of State government and administration. That of New York is taken as a type. This is just as full and comprehensive as the preceding part, except in the case of cities. They are very scantily treated in two pages, whereas at least twenty-five would be necessary to make clear their organization and relation to the county and State governments. The subject of education and school organization is amply treated. We lay down the book with a feeling of profound satisfaction, and with full appreciation of its value as a book of reference.

The author's descriptions are impartial, and he rarely presents his own opinions or views. When he varies from this rule, his success is such that we are tempted to wish he did it oftener. Witness this comment: "There is need for a national bureau of immigration and naturalization, in which shall be kept a register recording the names and full particulars of every immigrant, and also a register of every immigrant naturalized under the laws of the United States, and which shall refer to the entries in the other register."

We commend the book unreservedly.

Organic Analysis. By ALBERT B. PRESCOTT. New York, Van-Nostrand. 8°.

THIS volume is in a measure an outgrowth of the useful little book put forth by the same author thirteen years ago under the title of

'Outlines of Proximate Organic Analysis,' and deals mainly with certain common organic compounds of importance in commerce or pharmacy. Many topics which are touched upon in the smaller book — such, for example, as the properties of the alcohols and alcoholic derivatives, and of the carbohydrates — are here passed by, excepting, perhaps, mere incidental mention; but such substances as are discussed at all, are in general treated fully and exactly, with liberal citation and reference to authorities. The alkaloids naturally hold an important place; and schemes for plant-analysis, the examination of coloring-materials, and the separation and identification of fats and oils, are prominent. The title of the book is suggestive of a view broader than that actually presented, but it should be said in this connection that information upon the more important topics omitted is easily accessible elsewhere. This book is a decidedly valuable contribution to the literature of analysis.

Elementary Chemistry. By M. M. Pattison Muir and Charles Slater. Cambridge, Eng., University Pr. 12°. $1.25.

Practical Chemistry. By M. M. Pattison Muir and Douglas Carnegie. Cambridge, Eng., University Pr. 12°. 80 cents.

These two books are complementary, and together outline a progressive course in elementary chemistry.

The 'Practical Chemistry' leads experimentally from the demonstration of the distinction between simple physical and chemical changes up to such topics as the investigation of atomic weights, the phenomena of dissociation, the relative affinities of acids, the constitution of compounds, rates of etherification, and specific volumes; the acquisition of the elements of qualitative and quantitative analysis being assumed as an intermediate and outside incident of the course of work. The 'Elementary Chemistry' presents the essential facts and theories of chemistry, carefully distinguished and correlated in a clear and logical manner, the properties of bodies being discussed in the light of the 'periodic law.' The plan of instruction is in many respects unique and admirable, and reflects very strongly the growing tendency toward the early introduction of methods approximately quantitative.

Down the Islands. By William Agnew Paton. New York, Scribner. 8°. $4.

The author, who made a brief voyage to the Caribbees and British Guiana, tells the experiences and observations of his voyage. In an introductory note he confesses that on starting he had no knowledge whatever of the country he was going to visit. If this be true, he has made good use of his brief trip, for the book contains much valuable information; not the less valuable, as told in a very attractive form. In reading the description, it would seem as though the writer gives nothing but the impressions of an observant traveller who is unexpectedly taken to a world entirely new to him; and this makes his tale very charming. His remarks show that he is quick to catch the characteristic features of the country he visits; and his descriptions of the character of the several islands, of the English and French Creole, of the negroes, the 'black and yellow Caribs,' and of the Hindu coolie, are worth reading, Besides, a considerable amount of reliable statistical and historical information is embodied in this book, which gave us greater pleasure and satisfaction than many a pretentious book of travel.

Under the Southern Cross. By M. M. Ballou. Boston, Ticknor. 12°. $1.50.

The author, who has spent much of his time in travelling all over the world, tells in the present volume the story of a journey to the Pacific Ocean. Starting from Boston, he crossed the continent, and began his sea-voyage in San Francisco. A few days were spent on the Hawaiian Islands, a few hours' stay was made at Samoa, and then he proceeded to New Zealand and Australia. The time has passed when scientific results of great import may be gleaned from such a journey; but the author tells in an attractive form his observations and experiences, and gives us a glance of the life of the colonists and natives of the Pacific Ocean so far as he has seen it.

Special attention is devoted to the political relations of the South Sea colonies to America and Europe. The author dwells upon the question of the proposed federation of the Australian colonies and the probability of their becoming an independent republic, upon American influence in Hawaii and the development of American trade on the islands in consequence of the reciprocity treaty, and upon the late events in the Samoan Islands. Australian stock-raising and mining, and British immigration to these countries, are discussed, as well as the influence of the Chinese and of coolie labor, but the main and best part of the book are the interesting sketches of cities. Several descriptions of scenery are vivid and attractive, but those passages in which the author attempts to touch upon questions of geography or ethnology show that he has only paid a flying visit to the Pacific Ocean, and that he has not lived long enough in those regions to gain a thorough insight of their nature and of their natives.

NOTES AND NEWS.

The *Railway Review* says that the Russians are pushing forward the Transcaspian Railroad as rapidly as possible. Seven thousand men are now grading the road through Bokhara. It is now ready for the rails for four-fifths of the way between the Oxus and Samarcand, nearly three hundred miles; but the track cannot be laid until the bridge over the Oxus is completed. This bridge, now more than half finished, will be three miles long. It will connect the road now completed to the Oxus with the extension to Samarcand, and next spring the line will probably be in operation.

— It is but a short time since we called attention to Edwards's 'Butterflies of North America,' and now a new part lies before us. Indeed, within a twelvemonth four parts of the new series have appeared, the intervals between them being briefer than has been the case with any preceding numbers in the twenty years it has been running. More species of the prolific genera *Colias* and *Argynnis* are figured, but the specially attractive plate of the number — and there is always one — is that devoted to *Cœnonympha california*, or *galactinus* as Mr. Edwards would prefer to have us call it. The transformations of this genus are now for the first time illustrated by the early stages of one of our American forms; and the number of exquisite figures given to these early stages would be deemed almost luxurious if we were not accustomed to this kind of generosity on Mr. Edwards's part. The species is abundant on the Pacific coast, but was first raised in West Virginia from eggs sent the author from California, and we now know its history better than any species of the genus is known in Europe. Two forms, distinguishable by slight differences in the intensity of the markings, have long been regarded as one and the same species; but it was reserved to Mr. Edwards to prove by his precise experiments that the two were seasonally dimorphic forms of one and the same species, the darker giving birth the same summer to the lighter. We wish that this work, so great a credit to American science and American art, were better supported, and not published at so heavy an expense to its indefatigable author. It is in fact superior, both in matter and in execution, to any thing which is done abroad, and ought to receive ample support at home. Yet we chance to know that nearly forty per cent of the regular subscribers to the work come from outside of the United States. This shows, indeed, its appreciation in other countries; but it is a kind of work which should be found in every considerable library of the country, as a stimulus and an aid to workers young and old, and to show what one man, remote from associates, libraries, and even from much of his own field of work, may accomplish therein.

— Gardiner G. Hubbard, C. E. Dutton, O. H. Tittman, J. H. Gore, C. H. Merriam, J. R. Bartlett, R. Birnie, jun., J. W. Powell, Henry Gannett, A. H. Thompson, A. W. Greely, Henry Mitchell, George Kennan, Marcus Baker, and Gilbert Thompson, all of Washington, have incorporated the National Geographical Society for a term of one hundred years. Its principal objects are to increase and diffuse geographical knowledge, to publish the transactions of the society, to publish a periodical magazine and other works relating to the science of geography, to dispose of such publications by sale or otherwise, and to acquire a library under the restrictions and regulations to be established by its by-laws. The officers elected for the current year are as follows: president, Gardiner G. Hubbard; vice-presidents, H. G. Ogden (United States Coast and Geodetic Survey), Com. J. R. Bartlett (Hydrographic

Office), Gen. A. W. Greely (chief signal-officer), Dr. C. Hart Merriam (Department of Agriculture), A. H. Thompson (United States Geological Survey); treasurer, C. J. Bell; secretaries, Henry Gannett (United States Geological Survey), George Kennan; managers, Dr. J. C. Welling (president of the Columbian University), W. B. Powell (superintendent of schools, Washington), Capt. Rogers Birnie, jun., U. S. A., W. D. Johnson (United States Geological Survey), Henry Mitchell (United States Coast and Geodetic Survey), Marcus Baker (United States Geological Survey), G. Brown Goode (National Museum), Cleveland Abbe (United States Signal Office).

— 'Little Poems for Little Children' and 'Stories for Little Readers' (Chicago, Interstate Publishing Company) are books of elementary reading for students in primary grades. They are considerably above the average of such books.

LETTERS TO THE EDITOR.

**** *Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

Twenty copies of the number containing his communication will be furnished free to any correspondent on request.

The editor will be glad to publish any queries consonant with the character of the journal.

A New Meteorite from Texas.

WE have this day received a new entire meteorite from Texas, weighing about two hundred and eighty pounds. It belongs to the class siderolites, although the nickeliferous iron apparent to the naked eye is scarcely more than in some of the aerolites. Olivine is present in great abundance, giving a yellowish-green appearance to the whole mass. A hasty examination also reveals anorthite and a few specks of a bronzy looking metal, which is doubtless noilite. The meteorite was brought to us by one of our assistants, who found it near the south-west bank of the Colorado River, about three miles south-west of La Grange, Fayette County, Tex.: we would therefore suggest the name of 'The La Grange Meteorite' for it. A fuller description, with complete analyses, will be published later. WARD & HOWELL.

Rochester, N.Y., Jan. 31.

Jacobson's 'Higher Ground.'

YOUR notice of 'Higher Ground' in *Science* (x. No. 254) was so kindly, that I hesitate to impose upon your good nature by asking you to devote additional space to the subject. And whatever I may say will not be said in a spirit of controversy.

You approve of manual training in public schools, and you approve of the succession-tax as a means of enabling all children to get the benefit of the schools. Your only question is, Would the proposed succession-tax pay the bill? and your answer is, that it would not.

If a change so great as the one proposed could be made all at once, the proceeds of the succession-tax would not be sufficient to pay the bill. But it would take years and years to bring about so vast a change; and I believe that the proceeds of the succession-tax would be sufficient to pay the bill as fast as the change could be brought about, because wealth is increasing much faster than population. As an illustration of a change to which there is comparatively little opposition, see the length of time it takes for the high-license movement to make its way, — a movement full of good sense, to which, from pecuniary interest only, the liquor-dealers are opposed. What would not the opposition be to the succession-tax movement, and the apparent absurdity of paying people for keeping their children at school?

To say that there were in this country, in 1880, 8,347,731 children of the age in question; and that to pay, at the rates proposed, three-fourths of their number for going to school, would require $919,502,737.50; and that this sum could not be raised by the proposed tax, — is not that very much as if some one had said in 1830, "To do the transportation business of this country, we shall need 140,000 miles of railroad, costing eight thousand millions of dollars, and such a sum could not be raised for such a purpose"?

The money for the railroads has been found, because it has been found that railroads develop and enrich the country; that the money spent for railroads comes back, and comes back a hundred-fold.

The money for the education which I propose will be found when the people shall become convinced, that, invested in improving the brains of the people, — the motive power of all motive powers, — it will be more profitable than money invested in railroads or in any other enterprise whatever; that the money spent will come back, and come back a hundred-fold.

If in 1830 any one had predicted that in 1888 we should have our present mileage of railroads at its present cost to the country, he would have been laughed to scorn, because such an expenditure for highways must then have appeared absurd to the average man. But we spend all this money for highways, and thrive by it.

The figures in 'Higher Ground' are only tentative, re-adjustable at every point. Any public body into whose hands the practical working should fall would of course cut its garment according to the cloth on hand. My proposition is, that children shall be paid for going to school from twelve to twenty years of age, and that the amount to be paid for the eight years shall be $1,200. But if only money enough could be raised to keep them at school till eighteen, then the pay must cease at eighteen. That would require, in all, only $575 for each child. If at first only enough could be raised to keep the children at school till sixteen, then sixteen must be the limit. That would require, for the four years of each child, only $250. Even then the gain of the people in intelligence and efficiency would be immense, and the expense for the four years would be $250 only, instead of $1,200 for the eight years.

My proposition is, that all children from twelve to twenty years of age shall be paid for going to school substantially what they could earn out of school: at the age of twelve to thirteen, $50; thirteen to fourteen, $75; fourteen to fifteen, $100; fifteen to sixteen, $125; sixteen to seventeen, $150; seventeen to eighteen, $175; eighteen to nineteen, $225; nineteen to twenty, $300.

This, I think, would keep the children at school, and we should have an intelligent and efficient population, such as the world has never yet seen. Perhaps a trifle less annually would keep the children at school. I should be in favor of the smallest amount possible that would accomplish the object. But of course this could not begin all at once all over the country. If the proposition shall ever be carried out anywhere, it would take years and years after the beginning before all parts of the country would adopt it. All the children would not go. Wealthy people would still prefer to send their children to private schools; perhaps some Catholics, not many, would persuade themselves that the supposed interests of their children in the next world demand their absence from the American public school; and there are perhaps people among us so shiftless or degraded that they would not send their children to school, no matter what the inducement.

It is not necessary that I should be able to show that we could to-day provide for a state of things which can only be brought about after years of agitation. The state of things which I advocate can only come about gradually. The people will have to be convinced. Schoolhouses will have to be multiplied, and these things can only be done slowly and gradually, That the tax would be sufficient to begin with in large cities, there can be no doubt; and, as wealth increases more rapidly than population, the proceeds of the tax would tend constantly to come nearer being sufficient than it would be to begin with. In discussing matters of taxation, the *Chicago Tribune* said a few days ago that there are five hundred millionnaires in New York City: there were probably not fifty millionnaires in New York twenty years ago. There are probably one hundred millionnaires in Chicago to-day: twenty years ago there were not five. Smaller fortunes are increasing in proportion. Wealth is increasing much more rapidly than population.

No man can tell what the succession-tax would yield: it can only be found out by experiment. Did we not lower the tariff in 1883 to decrease the surplus, and then find that we had a steadily increasing surplus? I do not pretend to be able to calculate what the succession-tax would yield in the whole country, nor in any one state or city. On p. 44 of 'Higher Ground' I gave it as an estimate that the tax would yield annually from three to six millions in Chicago, and from twenty to fifty millions in New York. To this estimate I still adhere. The many large estates falling in from

time to time show it to be a moderate one. The tax would enable us to begin, and every year it would prove more nearly adequate: every few years we should be enabled to take in children of a more advanced age. The *New York Times* of Jan. 13 gives a summary of the comptroller's report of the State finances for 1887. The collateral inheritance law yielded for the year $561,716.23. The comptroller says it might easily in some years produce a million, and yet under that law no lineal inheritance is taxable. The greater part of the money came from eight estates: estate of Henrietta A. Lenox, New York, $76,534.27; estate of Mary J. Morgan, New York, $64,201.64; estate of Cornelia M. Stewart, New York, $61,232.03; estate of Calvin Burr, New York, $39,711.46; estate of Hannah Enston, Kings County, $40,068.20; estate of Sarah Marrow, New York, $14,077.35; estate of Mary E. Miller, Orange County, $15,796.65; estate of B. F. Bancroft, Washington, $10,419.60. This tax, being on collateral inheritances only, reaches only a small number of successions.

I speak of the apparent absurdity of subsidizing parents to keep their children at school. Several of my friends are at the present time supporting boys in manual-training schools. These friends of mine are not doing any thing absurd, are they? No, they are doing an excellent thing for the boys. Many colleges give aid and assistance to students. To do what I propose would be only doing what the colleges have always done, and are now doing, to the best of their ability, — helping indigent students to get an education. There is nothing absurd about that, is there? Why should it be absurd to do for all what it is wise to do for the few? Besides, the education itself would immensely accelerate the acquisition of wealth, just as the small beginnings of railroad-building from the thirties to sixties helped to accelerate the increase of wealth sufficiently to give us the railroad mileage of 1888. What the world has acquired in the way of knowledge would be known to all, instead of being known only to the few: all, instead of only the few, would have access to, and would utilize, the world's stock of knowledge, and the difference this would make in the production of wealth cannot be estimated. Where there is now one millionnaire, there would be a thousand of them under the new state of things, and all the people would be in comfortable circumstances. That increase of knowledge brings increase of wealth must be clear to every one. If, instead of our present population, we had a land full of Russian Moujiks, or of people born in Spain or in Arkansas, we should not be troubled with a surplus.

The education which I propose means that no child shall go through life in the raw state; that every child shall be a finished product; and that society shall get upon every human being born the profit of the finished product, instead of such profit as there is in letting humanity go through life in the raw state, as it were.

The world is wasting its knowledge by confining it to so few. It is as if a man were to leave his family a million, and provide that only a hundredth part of it should be put out at interest to produce income. We should call such a man foolish. Well, in like manner the world is stupid in confining knowledge to the few, and depriving itself of reaping the benefit of the service of the many in their best estate. Say that a man has five children and $100,000. He can educate his children well, and leave them $80,000; or he can let them go to school till they are twelve years old, and then leave them $100,000. Can any sane person doubt which would be the better course for the children? Can any one doubt which course would be the more likely to preserve the estate? Can any one doubt which would be the more likely to increase it?

But the children whose education I advocate have not the money to enable them to be educated, and their parents have not the money wherewith to educate them. Must the rich educate the poor? I say yes; if the rich wish to live in comfort in a country governed by universal suffrage, they must do their share, and more than their share, to educate everybody. As I believe, the people who would pay the money would get a handsome return upon their investment, even those who should pay at the highest rate.

Years ago I said, and I quote it here from Prof. C. M. Woodward's recent book, 'The Manual Training School,' published by Heath & Co., —

"The alternative before you is more and better education at greater expense; or a still greater amount of money wasted on soldiers and policemen, destruction of property, and stoppage of social machinery. The money which the training would cost will be spent in any event. It would have been money in the pocket of Pittsburg if she could have caught her rioters of July, 1877, at an early period of their career, and trained them at any expense just a little beyond the point at which men are likely to burn things promiscuously. It is wiser and better and cheaper to spend our money in training good citizens than in shooting bad ones."

The first requisite is to convince the people that the thing itself is worth doing. That done, the means to accomplish it will be found. The thing proposed "is not a largess to the recipient, but a natural measure of self-defence on the part of the government which educates."

I propose it as a measure for the welfare of the community, and the welfare of the community is the supreme law.

Once established that it is the height of wisdom at all hazards and at any cost to bring the children into school and keep them there till the twentieth year, if necessary other means besides the succession-tax would be found to pay the expense. The $500 license-tax on saloons yields annually in Chicago nearly two millions. It is a new revenue never before counted upon for municipal purposes. Before we had it we got along very well without it, and we could again do so. To what better use could the license money be put than to keep the children at school? And the tax might be doubled. Double our rate, and liquor-licenses would annually yield in New York City something like ten millions. Then there is the internal revenue derived from tobacco and whiskey, yielding annually over a hundred millions, which is every day in danger of being abolished because we have no use for the revenue. This tax, unless seized upon for education, is liable at any moment to be repealed. Its appeal would be a calamity. The tax bears heavily only upon vice and crime. No useful industry is hampered by it. There is not one single good reason why it should be repealed. To what better use could the proceeds of this tax be put than to be paid out for keeping the children at school? The whiskey and tobacco tax might be doubled, and nobody be the worse for it. It is low now in order that it may not produce too much revenue. If the revenue were needed for a good purpose, the tax might well be doubled and yield over two hundred millions.

In the sense in which I speak of the settling of the labor-troubles, they would be settled if we could get along without periodically employing soldiers to use force. The graduates of the manual-training school would be just that many people taken out of the labor-problem; and, if the number so taken out was sufficient, there would be no labor-problem left.

Each individual trained to a degree to find an independent way for himself instead of relying merely upon the work of his hands to be directed by the brains of some one else, is to the extent of that individual a settling of the labor-troubles. The settling would operate as things did in Germany in the time of the first Napoleon. So long as German soldiers fired their guns at his command upon his enemies, he maintained his supremacy in Germany; but when the Germans took to shooting at him and his, instead of for him and against his enemies, there was end of Napoleon's supremacy. Sufficient training, intelligence, and efficiency would make all our people for peace, and there would therefore be peace. The lawlessly disposed would be so few and lonesome that they would cease to riot. If I may be allowed an Irish bull, the lawless could be made to shoot the other way by being made so intelligent and efficient that they would refrain altogether from shooting.

AUGUSTUS JABOBSON.

Chicago, Jan, 24.

Weather-Predictions.

IF Professor Hazen is willing to admit, as I infer from his letter in *Science* of Jan. 27, p. 49, that the Blue Hill predictions for last October give a higher per cent of success than his own when verified by the unmodified original rules he sent me, it seems to me there is an end of the matter between us. I do not deny that better methods of verification of weather-predictions are wanted. All that I have ever claimed is, that the Blue Hill predictions, when verified by the Signal Service rules, in accordance with which they

were made, give a higher percentage of success than the Signal Service predictions for this vicinity. Professor Hazen made the predictions for the Signal Service during October; and if more extended comparisons between his predictions and those of Blue Hill are of importance, why not compare the Blue Hill predictions with the similar predictions of the Signal Service, published in the same newspapers? The Blue Hill predictions were made for south-eastern New England, and I am perfectly willing that they should be verified for the States of New Hampshire, Massachusetts, and Rhode Island in accordance with the published rules of the Signal Office (see chief signal-officer's report for 1886). In making the Blue Hill weather (not temperature) predictions, the phraseology and definitions of the Signal Service have been closely followed; and, if any of the readers of *Science* care to extend the comparison, I will gladly furnish them with the past or future Blue Hill predictions as they appear in the Boston papers, since I am confident that these, when verified in accordance with the published Signal Service rules, will give a higher percentage of success than the predictions of the Signal Service. When it is considered that the Blue Hill predictions are extended for nine hours longer in advance than those of the Signal Service made from the same telegraphic reports, and that less than one-third the telegraphic data at the command of the Signal Service are available at Blue Hill, it seems clear that by improved methods and more localized predictions the efficiency of the Signal Service could be greatly improved and its expenses reduced. During January the Blue Hill predictions will average something like fifteen to twenty per cent higher than the Signal Service predictions for this locality; and this seems of interest, since I understand that Professor Hazen, who is assumed to be one of the leading predicting-officers, made the Signal Service predictions for this month. H. HELM CLAYTON.

Blue Hill Observatory, Jan. 30.

Hybrid Diseases.

IN a paper presented at the recent meeting of the American Public Health Association (*Science*, x. 289), Dr. E. M. Hunt of the New Jersey Board of Health brings out some original ideas about disease-germs, that are likely to prove misleading to persons whose knowledge of the subject is derived from the public press. The etiology of so many zymotic diseases is now under investigation by experts in bacteriology, that the general reader or practitioner who is not an investigator is severely taxed to keep track of the often conflicting and incomplete results; and an especial effort should be made to avoid unnecessary complication of the subject by the introduction of theories not based on a correct understanding of what is known or extremely probable.

Excluding the protozoan claimed by Laveran and others as the cause of malarial fever, the moulds that occur in connection with certain local diseases of the ear, etc., and the *Actinomyces* of man and some other mammals, the active agents of common parasitic diseases that are at all credited are bacteria. One of the systems of classification now generally used recognizes four main divisions of lower plants below mosses and liverworts, — thallophytes, zygophytes, oophytes, and carpophytes, — beginning with the lowest. Bacteria fall by common consent into the first and lowest of these groups, — the protophytes. This group is a sort of *omnium gatherum* for many things that cannot be placed elsewhere, and is chiefly known by negative characters, the absence of much evident structural differentiation, and of any form of sexual reproduction, heading the list. This being the case, it would partake of dogmatism to make any very emphatic assertions about the plants that now find lodgement in it; yet it may fairly be said that no theory that rests upon the assumption of sexual processes in any of the protophytes is tenable. Hybridity is usually the result of sexual union between representatives of two more or less nearly related species, and in this sense is not only not known among plants of this group, but very improbable, since they have thus far given the best investigators no indication of even the simplest form of sexual union, — conjugation. The only other mode of hybridizing, if it really be such, corresponding to the formation of 'graft-hybrids' among flowering plants, could come only from the fusion of individuals of two species, and would amount to conjugation. It seems to me, therefore, that such a theory of hybrid diseases as Dr. Hunt has propounded is entirely untenable, and a very unfortunate addition to a literature already overcrowded with notions that others must eliminate.

I fear that my friend Mr. Meehan wrote his opinion on lichens rather hastily, and perhaps without intending to have it given to the readers of *Science*, or he would scarcely have expressed the belief "that all lichens are hybrids between fungi and algæ." Botanists do not agree on the lichen question, any more than physicians do on the germ-theory of disease; but neither the followers of Schwendener, nor the old school, would be likely to advocate the hybridity that Mr. Meehan believes to be conceded. The relationship of the two parts of a lichen, according to the Schwendener school, is merely that of association, either parasitic or symbiotic, and in no sense comparable to hybridization, while the advocates of lichen autonomy hold them for parts of one and the same individual.

Realizing fully the advisability of excluding dogmatism from the discussion of all that pertains to sanitation, I have written this correction in no *ex cathedra* spirit, and I trust that it will not appear to either Dr. Hunt or Mr. Meehan as any thing more than an effort to check the entrance of error into the discussion of one of the most important subjects that is prominently before the public.

WILLIAM TRELEASE.

St. Louis, Mo., Jan. 28.

Color-Blindness.

REFERRING to your comment in *Science* of Jan. 27, I would say that I have always believed that the defect of color-blindness could be accurately described only by one who, like myself, is subject to the peculiarity. From an early age I have been aware of the trouble, and by my attempts to assign names to colors have often furnished my friends much amusement. I have made many efforts to correct the defect, and am convinced that any attempts to educate the color-sense will result in no benefit to those who are really color-blind.

There are two sets of colors which in my mind will always be hopelessly confused. The greens, browns, and reds comprise the first; and the blues, pinks, and purples, the second. None of these colors seem to me absolutely alike. The contrast, however, is not striking, and I should describe each of the three as different shades of the same color.

Being near-sighted, I could not at a distance distinguish the blossoms from the leaves of a bed of scarlet geraniums. On approaching, however, I could readily detect the difference, but should describe the flowers as darker than the leaves, though to my eyes somewhat similar in color. While riding through the fields of France, members of our party frequently exclaimed at the multitude of scarlet poppies in the grass. Though I looked with longing eyes, not a poppy did I see during the entire journey. Similarly I am unable to detect cherries upon the trees, or strawberries on their vines, unless quite near to them. Notwithstanding this confusion of green, red, and also of brown, I can, by the worsted test, detect a difference in all the shades of these three colors. If I attempted to assign names to the various hues, it would of course be mere guess-work. The neutral tints of a November landscape, too, possess great beauty for me. The green of the grass, the browns of the leafless trees or of the soils in adjoining fields, the sombre hues of the sky, are all pleasing to my eye. Such being the case, the term 'color-blindness' seems altogether a misnomer.

The second set of colors I should describe as follows: pinks, blues, and purples are closely allied; I should call them all blue. Pink seems a lighter, and purple a darker, shade of the same hue. But, as in the case of the first set, all variations of these three colors are readily manifest to my eye.

It may seem too strange to be true, but I have frequently arranged flowers into bouquets which have been perfectly satisfactory to those who are not color-blind. I have, of course, no means of determining whether a brilliant sunset is more charming to others than to myself. I fancy that my defect deprives me of very little of its beauty.

Although in the rainbow I can distinguish only the red, yellow, and blue, it is probably as attractive to me as to others. I have as

yet failed to find any one who can readily detect the seven primary colors. It is said of Dalton, from whom color-blindness was once named, that he could distinguish only the colors of blue and yellow in the solar spectrum. Dr. Mitchell tells of an officer who chose a blue coat and a red waistcoat, believing them to be of the same color; of a tailor who mended a black garment with a crimson patch, and put a red collar on a blue coat. Such mistakes seem quite as ridiculous to me as to others. Yellow and black I have never confounded with other colors.

There is such a diversity in color-blindness, that it seems impossible to determine the cause. I am convinced that it is a physical defect. The eye, as a mechanical instrument, has not been found at fault. The cause is undoubtedly due to some peculiarity of cerebral formation. Like the cause of left-handedness, which is due to unusual development of the right brain, color-blindness is due to a freak of nature.

The education of the color-sense among the children of the primary schools has proved of great value in removing that uncertainty in distinguishing colors which of course may be found among most ignorant people, old or young. This has its parallel in the education of the ear to the appreciation of all the variations of the musical scale. But for one who is really color-blind, education can be of little avail in correcting the defect. W. B. HARLOW.

Syracuse, N.Y., Jan. 27.

A New Text-Book on Zoology.

THERE can be no better evidence of the growing interest on the part of certain reading-classes of all ages, and the importance that is being daily attached to biological studies by school authorities and educators, than the ever-increasing demand for good text-books in zoölogy, and the frequency with which such volumes put in an appearance. We now have before us a thoroughly revised edition of Steele's 'Fourteen Weeks in Zoölogy' (New York, Barnes), — a little work that held its place with great popularity for ten years, and which has now been almost entirely rewritten by Prof. J. W. P. Jenks of Brown University, who is quite responsible for its present form.

From the author of the work I learn that the volume in scope is principally designed for beginners in our high schools and academies at the average age of fifteen to eighteen years, in which schools they have no special means for illustration. Moreover, to be efficient as a text-book, it is intended to be used only by a class of teachers who presumably possess quite a thorough knowledge of general zoölogy, drawing, dissecting, zoölogical aids and appliances, and kindred subjects. Taken as a whole, were this volume placed in the hands of such a teacher, and its chief aim to be to impart a notion of general zoölogy to a class of students of the average age mentioned, after faithfully following out its chapters for three or four months, we must believe that no better work has yet appeared having a higher claim to such an end. Its pages are crowded with beautiful cuts of the forms used in illustration of its text, which cuts and illustrations have been for the most part admirably chosen; and, notwithstanding its unavoidable brevity, the subject-matter, as a rule, is presented in a manner calculated to interest and instruct the student at every step. It seems to me, however, that even in a work of this character its author should add a page to his preface, and explicitly state in words and figures and acknowledge to whom he is indebted for his illustrations. We find here numerous drawings of birds taken from Audubon and Wilson, and many others, without a word of such acknowledgment, and the oversight occurs throughout the work. We must believe that even young academical students should be taught that this is not the proper custom; but where an author meets with such material assistance, it should be duly noted. An excellent feature of the work consists in properly dividing and accenting the technical names to assist in their pronunciation; while, on the other hand, a serious defect is evidenced in the absence of a 'glossary of terms' at the end of the volume.

In the main, the classification adopted shows the impress of recent views in the premises; but here, as much as anywhere else, it needs the explanation of a skilled teacher, as the student would gain but a very erroneous idea of the subject from this work alone, as no family nor generic lines are drawn. Take, for example, the order *Passeres*, where lyre-birds, birds-of-paradise, finches, crows, and larks, follow each other in the order I have given them, without a single word of explanation as to their affinities. Then again we find the author at total variance with the leading authorities in placing the bats in the order *Insectivora*, without a word as to why such a step should be taken. Nor will he meet with full support in his order *Bimana*, containing only "one genus and a single species," and that species having "the rank of a being who is alone declared to have been created in the image of God" (p. 277). We have no scientific proof for this latter view. Beneath about half the figures we find given in parentheses each one's proportionate size as compared with the living subject: we regret that this excellent idea was not carried out through the entire work, and it will be well for future text-books in zoölogy to adopt this plan. Written, as the author of this work declares it is, for a class of students as late as eighteen years of age, to my mind it exhibits another thoroughly fatal omission, for it has not a word to say of that great universal law pervading all nature and the world, which explains the very origin of organic forms and the relations of the living ones to those now extinct. Should a young man of eighteen years of age complete the course pointed out by this work, and yet be ignorant of the law of evolution, I hold his zoölogical studies have been but poorly grounded. A companion work to the one under consideration on physics would be in the same case, had it omitted the law of gravitation.

The object of a text-book in zoölogy for a class of students from fifteen to eighteen years of age should not have as its aim the endeavor to teach the greatest number of names of animate objects, for at the present day that is a hopeless task, even were it a desirable end. It should, on the other hand, undertake to make clear the general principles of biological classification; it should by a careful, detailed study of a few types, both vertebrate and invertebrate, clearly point out the universality of morphological laws, then these two lessons should be combined; next, it should be clearly shown the relation between living and extinct types, and finally, by a few clear examples, show the origin of certain forms, as the birds from reptiles, and the ancestry of the horse, and so on; all of which is far more comprehensible than a jumble of isolated facts unconnected by any known law. Such a course, properly expanded and illustrated by a competent teacher, will give a student at once a more intelligent appreciation of life and living forms; make him a better observer; create in his mind a more healthy interest in the subject; and finally send him forth with a kind of stimulation and systematized knowledge which fits him to further pursue biological research, should it happen in any given case to be imparted to the mind of a student cast in the biological mould.

R. W. SHUFELDT.

Fort Wingate, N. Mex., Jan. 9.

The Flight of Birds.

IT is with great diffidence that I take part in a discussion participated in by such eminent authorities as Professor Newberry and Professor Trowbridge, and it is with still more hesitation that I venture to disagree with any opinions brought forward by either of these gentlemen. Nevertheless, I can but feel that undue stress has been laid upon certain facts, while others of equal importance have been overlooked or incorrectly stated.

To a great extent the discussion hinges on the assumption that birds need some mechanical device to relieve the muscles of strain while soaring, — an assumption whose truth seems open to question, as many of the lower animals are capable of automatic muscular movements of very long duration.

Among mammals the cetaceans are almost constantly on the move both by day and by night, while others rest in positions that seem to entail considerable muscular strain. Thus horses very frequently sleep in a standing posture, and the skunk and baboon have been observed to seek repose lying flat upon their backs, with all four legs stiffly extended in the air; a very good example of unrelieved muscular strain may be seen in the tail of the spider-monkey, whose prehensile power is sufficient to sustain the animal after life is extinct; some birds, during their migrations, fly or swim for immense distances without stopping for rest, and there is very good reason for believing that many of the petrels keep on the

wing for days and nights together without intermission; many fishes require perpetual motion in order to preserve their equilibrium, while other pelagic forms appear to be on the move for long periods of time without flagging, — all these cases necessitating oft-repeated movements, which call for far more serious strain on the muscles than the mere extension of the wings during the act of soaring.

The strain on the extensor muscles at such a time can be but trifling, compared to the strain on the levators and depressors of such a bird as the albatross, whose weight of nine to fifteen pounds is supported by two levers of the third class, five to seven feet in length; and yet no bird makes longer flights than this wanderer of the southern seas, who has no special device to keep his wings outstretched.

These instances are brought forward, not to disprove the fact that a device to ease the muscles in soaring may not exist, but to show that there is apparently not the slightest need for it.

In regard to the interlocking of the primaries, which unquestionably takes place, is not this the result of their emargination, and consequent failure to glide smoothly over one another, rather than the end to be accomplished by this cutting-away of the feather toward the extremity?

This view of the case is borne out by the fact that the longer, more flexible ulnar border of the primary naturally gives at each stroke of the wing, thus catching in the radial portion of the feather immediately behind it, whether the bird wishes it or not.

Moreover, during the act of soaring, the wing is expanded to its utmost, and the tips of the primaries widely separated, while in a fresh specimen of *Buteo borealis* no locking is possible until the wing is partially closed. This would seem to be conclusive as regards the importance of the locking of the primaries as an aid in soaring; although there remain the facts that some birds who soar to perfection — such, for example, as gulls, cranes, storks, and the frigate-bird — do not possess emarginate primaries, while others, like some owls and flycatchers, have emarginate primaries but do not soar.

Professor Trowbridge's comparison of the wing to a flat card is hardly felicitous, and his statement that it would be in a state of unstable equilibrium but for the locking of the primaries would seem open to serious doubt.

One absolute requisite of a wing is that the anterior margin should be rigid, and the posterior border flexible, — a requirement which is met toward the extremity of a bird's wing by bringing the quill close to the radial margin of the feather, leaving a posterior pliable edge.

Now, if the primaries are interlocked, a rigidity is created toward the ulnar border of the wing, which would thus become more card-like and unserviceable than if the primaries did not lock.

A pertinent question that might be asked of Professor Trowbridge, is, Why, if the "long primaries present a serious resistance . . . when a bird is soaring," do all birds that soar or sail possess just such primaries, while the corresponding feathers in birds which do not soar are short?

One feature in the wings of birds pre-eminent for soaring abilities, e.g., the *Vulturidæ* and *Falconidæ*, has not been touched upon in this discussion, so far as I am aware; and this is the fact that when the wing is extended to its utmost, as it invariably is during soaring, the metacarpus and phalanges are not in line with the ulna, but are bent forward of it. By this arrangement some of the muscles and tendons that ordinarily act in flexing the wing are brought upon the dorsal surface of the bones, and thus have their power of flexion weakened, or possibly even made to aid in the automatic extension of the wing. If, now, a bird with wings thus spread be so killed that there is no perceptible shock or nervous start, the bird may remain with outstretched pinions and sail gradually downward, — exactly such a case as Professor Newberry describes.

In conclusion, I can but regret that I have no facts to adduce that will throw any light on the problem of flight, as it is far easier to find fault with any theory than to suggest a better, and purely adverse criticism must always seem more or less ungracious.

FREDERIC A. LUCAS.

Washington, D.C., Jan. 16.

Binocular Combinations upon Disparate Retinal Points.

EVERY one is familiar with the fact that Wheatstone and many subsequent investigators have explained the binocular perception of solidity by the theory of the 'fusion of images upon disparate points,' as they are called, in the retina. They have generally denied the original possibility of a monocular perception of solidity and distance; and hence, when certain plane figures were stereoscopically combined, the apparent solidity of the resulting single figure suggested its explanation in accordance with what had previously been supposed of the mathematical relation between combination and convergence. Thus Wheatstone's view may be illustrated by the following figure. It is well known that the stereoscopic combination of these figures, although making a plane image only upon the retina and representing only a plane surface externally, nevertheless produces the appearance of a solid body. Previous theories of vision had maintained that single vision took place upon corre-

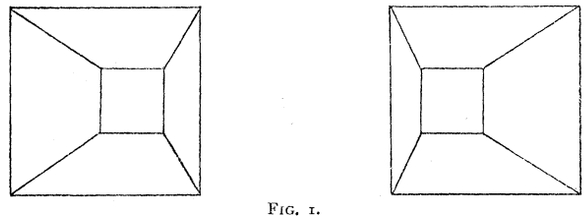

FIG. 1.

sponding points of the retina, and double vision upon disparate points. Now, as the mathematical construction of the case would not allow the inner figures and lines to fall upon exactly corresponding points, the apparently single character of the image in stereoscopic combination was most naturally explained by saying that fusion took place upon disparate points; and hence when the perception of solidity, or relatively different distances between the larger and smaller figures, uniformly accompanied this kind of fusion, it was naturally ascribed to that process as its cause. Whether such a fusion really takes place or not, has been hotly contested, and we wish here to present a few new considerations to show that it does not occur, notwithstanding the strongest apparent evidence of our actual perception of it.

To make the argument clear, a few words will be necessary upon what is meant by 'corresponding' and 'disparate' points. As indicated, they denote the points upon which respectively single and double vision takes place. But the second term has two very distinct applications, — one binocular, and the other monocular. It is

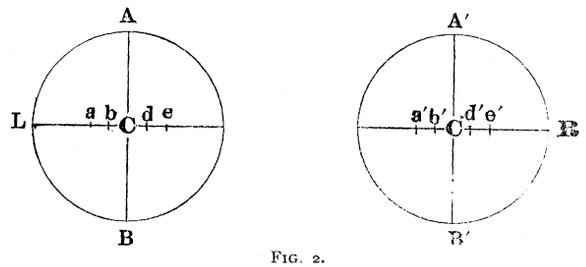

FIG. 2.

this last fact and its implications which most investigators, and among them Wheatstone, seem to have ignored. But the importance of taking it into account will be evident from the following considerations. Take the circles R and L to represent the retinal surfaces of the two eyes. Divide each retina into halves by the vertical meridians AB and $A'B'$. Draw also the horizontal meridians in which lie the points a, b, c, d, e, and a', b', c', d', e'; c and c', at the intersection of the vertical and horizontal meridians, represent the *fovea centralis* of each eye. Now, the vertical meridian divides each eye into halves, that correspond to the opposite halves of the other eye. Thus we have what are called the nasal or inner, and the temporal or outer halves of the eyes. The nasal halves of each eye are said to 'correspond' to the temporal halves of the other eye. How this will appear can be seen by superimposing one circle upon the other; and the points a and b in the temporal half of the left eye, L, will coincide with a' and b' in the nasal half of the right eye, R; and d and e in the nasal half of the left will coincide with d' and e' in the temporal half of the right eye. By

calling these 'corresponding' halves, we mean that they have the same function of localization; that is, that they are constructed for seeing the same object, at the same point in space, at the same time, assuming a given state of fixation and the proper position of the object. Thus one image of an object falling upon a in the left eye, L, and the other upon a' in the right eye; or upon b in the left, and b' in the right eye; and so on, — will make the object to appear single and in the same place. Hence they are called 'corresponding' points. But if one of the images falls upon a, and the other upon b' or any point between that and a', which may happen according to the position of the external object, there will appear to be two objects. This is because all other points than a' are 'disparate' in relation to a. So with b and c', or c and d'. Thus, while every point in a temporal half is a 'corresponding' point to a given point in the nasal half of the other eye, it is 'disparate' to all other points. This is the binocular use of the term. But since the temporal halves of the two eyes are non-corresponding halves, the points a, b, d', and e' are also 'disparate.' Now, in the monocular retina all the points are 'disparate' in relation to each other; that is, combination never takes place. Hence a, b, c, d, and e, or a', b', c', d', and e' are respectively 'disparate' in relation to each other monocularly considered. Then, since the temporal half of the right eye corresponds to the nasal half of the left eye, and the two are thus identical in visual functions, a and d', or b and e', are 'disparate' in relation to each other in precisely the same sense as a and d, or b and e, in monocular vision. This is the monocular use of the term. Now, since fusion of images never takes place in monocular vision (say, when separate images fall upon a and b, b and c, or a and d, and so on), it can never take place when the two images fall upon non-corresponding halves of the retina (say, both upon the temporal, or both upon the nasal halves; that is, upon b and d', a and d', or b and e'), any more than they would upon b and d, a and d, or b and e, and so on. The reason for this is plain. Each eye forms binocularly only half an eye, so that the temporal half of one is identical in function with the nasal half of the other. This being the case, the non-corresponding halves of the binocular eye form a monocular eye. Experiment will show this to be the case. Hence stereoscopic images falling both of them in the temporal, or both of them in the nasal halves of the binocular eye, will appear precisely as if one of them fell in the nasal and the other in the temporal half of the monocular eye, or as if both fell upon separate points in any one half of the monocular eye. Thus the images in the temporal halves a, b, and d', e', can no more combine than if they were a, b, and d, e'. Hence a, b, d', e', are monocularly 'disparate;' so also d, e, a', b'. Now, since monocular combination of 'disparate' images never takes place, we can demonstrate that it can never take place in stereoscopic combination; at least, where the figures to be combined are such as Wheatstone's original illustration represented: namely, two lines which indicate opposite inclinations to the median plane. This is shown in the following lines, where A and B, the upper ends of the lines, will fall upon temporal halves of the retina when C and D fall upon the fovea, and yet fusion is as apparent as if it were real. The lower ends fall upon the nasal halves, and fusion is also apparent; the total resultant being a line with the upper end nearer the observer than the lower, and apparently upright or at an inclination to the plane of the paper. But it is effected by non-corresponding halves of the eye.

FIG. 3.

To illustrate this, take Fig. 4, R and L representing the two eyes. Let A and B represent two figures farther from the median line EF than C and D. A and B may represent the upper ends of the lines in Fig. 3, and C and D the centres; both together forming a plane geometrical outline for a stereoscopic figure such as Wheatstone employed. Take E for the point of fixation before combination, so that we may suppose A, B, C, and D to lie in the horopter. c and c' are the *foveæ centrales;* and when the eyes are fixated for an object at E, its two images will fall, one upon c and the other upon c'; while those of A will fall upon e and e', of B upon a and a', of C upon d and d', and of D upon b and b'. Now, a and a', b and b', c and c', d and d', e and e', being corresponding points, the several objects will be seen single while they are in the horopter; but the position of their images upon the retina must be noticed before indicating the effect of stereoscopic combination. The images of each object fall upon corresponding halves of the retina; but the images of A and B compared, also of C and D compared, fall upon non-corresponding halves of the retina. Now, in stereoscopic combination the object is to make A and B, or C and D, appear to coincide respectively; that is, appear upon the fovea. This may be done by converging or by diverging the eyes. But this can be effected only by fusing one image of A or C in the one eye with the image of B or D respectively in the non-corresponding half of the other eye. By convergence the fusion will be of images at present in the temporal halves; by divergence, of images in the nasal halves: that is, by the former combination, must be of extra-foveal, and by the latter of intra-foveal, images. a and b, and d' and e', are extra-foveal, because they lie in the temporal halves: d' and e, and a' and b', are intra-foveal, because they lie in the nasal halves of the eyes. Now, if we converge the eyes so as to bring the image of C and D into the fovea, it is evident that the combination takes place only by what are extra-foveal images when the point of fixation is E. Convergence to produce combination of C and D requires a new point of fixation in the median line at the intersection of the lines which represent the course of light from C to d', and from D to b. When this is effected, the foveæ c and c' are shifted, the former to b and the latter to d', to receive the images at those points. But thus, while the images of C and D are fused in the fovea, those of A and B still fall upon extra-foveal points as far from the new position of c and c' as a is from b, and e' from d'. But being both extra-foveal, they fall in temporal and therefore non-corresponding halves of the retina. In Wheatstone's experiment, Fig. 1, these would represent the larger squares, and the apparent combination represented in the base of a visibly solid figure thus perceived is explained by 'fusion upon disparate points.' But being extra-foveal, and in the temporal or non-corresponding halves of the retina, these points are not binocularly, but monocularly 'disparate,' and hence the fusion claimed for them is as impossible as if it were claimed for the points a and b, or a and e, or

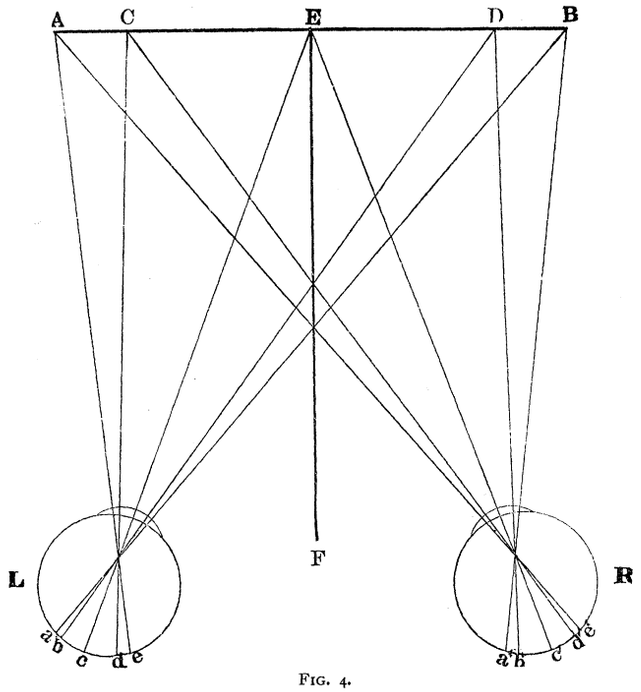

FIG. 4.

any two points in monocular vision. Images upon different sides of the fovea in monocular perception never combine, and are never supposed to combine. Now, supposing C and D in the foveæ e and c' by convergence, and keeping in mind the fact that the temporal half of the right eye in binocular perception corresponds to the nasal half of the left in monocular perception, the images of A and B, while they fall in non-corresponding halves, occupy positions visually the same as if they fell upon non-corresponding halves in monocular perception, the temporal and nasal; and hence, superimposing L upon R, e' would fall as far from the fovea in the nasal half as a from the fovea in the temporal half of the left eye L: that is, the images of A and B, a and e', visually fall upon opposite sides of the fovea, and can no more combine than separate images in monocular perception.

The same general result is obtained if we combine C and D by diverging the eyes; that is, by focusing the eyes in the median line beyond the point E, or beyond the stereoscopic figures. The eyes are thus turned outwards, so that the fovea in each case must be shifted inward from c to d, and from c' to b'. Combination of C and D will thus be attained by intra-foveal images, — such as are intra-foveal while the point of fixation remained at E. But when d and b' are brought into their corresponding fovea, e and a' still remain intra-foveal at distances from the fovea equal to that between d and e, and a' and b'. By the same argumentation as before, it can be shown that the images of A and B, respectively e and a', cannot combine. Thus, being both intra-foveal, they fall upon points in the nasal halves of the two eyes. These are binocularly non-corresponding, and therefore monocularly complementary halves of the retina: hence falling upon e and a' in binocular vision is the same as occupying opposite sides of the fovea in monocular vision, and so combination will be impossible. This shows the importance of observing what is implied by the term 'disparate.' As long as we conceive the term in its binocular application, there would be some reason for supposing combination upon them under the circumstances described. But adjustment by convergence and divergence, the former for extra-foveal and the latter for intra-foveal images, requires us to think of 'disparate' in its monocular application; and in that case we must either deny the possibility of combination upon them, or abandon the whole theory which makes a nasal half of one eye correspond to a temporal half of the other; for, if 'disparate' points in monocular perception may admit of combination, a nasal half may correspond to a nasal half, and a temporal to a temporal half, of the retina. This has never been assumed to be possible.

Of course, 'intra-foveal' and 'extra-foveal' are used with reference to the vertical meridian, and not the horizontal meridian, as Fig. 4 would seem to imply. In the last figure A and B represent positions relative to the vertical meridian of any objects in the temporal halves of the retina, and hence they may be above or below the horizontal meridian in which they really lie, according to the inclination of the lines to the median plane. The modification for the nasal halves of the retina can be supplied by the reader. It is evident from this that this demonstration does not apply mathematically to Fig. 1, where the apparent fusion is of binocularly 'disparate' points, although, taken in the *total* sense for *localization*, it will apply. But it is combination, not localization, that we are discussing.

If the stereoscope is used to effect the combination, the perspective noticed in convergence with the naked eyes is reversed, and is identical with that effected by the divergent movement to produce combination. The reason for this may be briefly stated. The partition between the lenses lies in the median line, and hence cuts off the extra-foveal images entirely. Combination has therefore to be effected by the intra-foveal. With this statement of the conditions, the argument could be carried out as before.

But the reply to our position that stereoscopic combination upon 'disparate' points must be impossible, will be the very plain one that it contradicts the *facts* of actual vision; that we can actually see the combination to have taken place; and, since it cannot have been upon corresponding, it must have been upon 'disparate' points. There are two replies to this, and, in addition, an important fact which explains the apparent anomaly. In the first place, the demonstration is mainly intended to show that the phenomenon must be impossible if we still retain the ordinary theory in regard to the divisions of the retina and their functions. In the second place, experiment shows that our claim is correct: for, after long practice in combination by convergence or by divergence, those images which, according to construction, must fall upon disparate points, and which at first seemed to be single and to coincide, appear double until they are brought into the fovea. This indicates that they were never really fused into one. Why, then, is the fusion so apparent to vision? The answer is, that inhibition had suppressed such portions of one or both images contending for fusion, that the resultant, made up of complementary elements, appears as a single image. After considerable practice, the reflex and automatic tendency is weakened, and inhibition correspondingly decreases; so that the images which before seemed single appear double, as the law of disparate points requires.

Baltimore, Md., Jan. 4. J. H. HYSLOP.

Bacteriology as a Study in Schools.

THE subject of the study of bacteria, discussed by Professor Conn in a recent number of *Science* (xi. No. 257), is one which deserves more attention than it has attracted thus far, and I take the liberty of making a few suggestions which have presented themselves to an investigator rather than a teacher, but which may prove useful to the latter. Let us call the subject 'bacteriology' for convenience' sake, and drop the misleading expression 'germ-theory of disease,' which has had its day. We know, as positively as we know that the earth revolves on its axis, that certain diseases in man and animals are caused by the invasion and multiplication of bacterial parasites. There is no theory about this. The phrase is misleading, because it states that all disease may be due to germs, which is manifestly untrue.

There are several classes of students who would be greatly benefited by a careful study of bacteria in the laboratory.

1. Students of general biology and physiology would gain by a few simple experiments, readily performed, a very clear insight into the great metabolic activity of life in general, of bacteria in particular. It would be easy to demonstrate the formation of soluble ferments related to pepsin and diastase; the production of soluble and insoluble pigments, and the effect of re-agents upon them; the relation of vital activity to oxygen as expressed by aerobic and anaerobic germs; the effect of bacterial growth on various substances, such as blood serum, gelatin, and milk; the resistance of spores to high temperatures; the effect of disinfectants and antiseptics; the phenomena of phosphorescence, nitrification, and other equally interesting and instructive features of bacterial life. The habit of close observation and careful differentiation may be cultivated by the parallel study of two species as nearly alike as possible. All this, and more, can be done with bacteria obtainable at any time, from natural waters, from the soil, the digestive tract of mammals and other animals, from milk and various infusions. To impress the mind with the destructive effect of pathogenic forms, a rabbit, or mouse, or guinea-pig may be inoculated with some germ fatal to these animals, but harmless to man. Such a form, fatal to rabbits, is occasionally present in the mouth. The microscopic study of bacteria brings out facts of histo-chemistry, and features of the microscope itself hitherto scarcely known, which should be applied in ordinary histologic work.

2. There is another class of students who stand in need of such instruction. Much of the preparatory work of the student of medicine can and should be done at our higher institutions of learning. For instance, the admirable work done at Cornell University in preparing students for the study of medicine, of which I have personal knowledge, has always tended to push students into the front rank at the medical schools. These have no time to spare to teach students how to dissect well, how to study anatomy or to acquire the methods underlying histologic work and chemical analysis, nor have they the time to teach bacteriology. Yet no one should graduate in medicine to-day who does not know something about the secret working of this miscroscopic world, who cannot reason with it in his practice, or recognize the different forms when a diagnosis may be based upon them. Our biological laboratories may do much to help the medical schools in this direction. The physician will then be equipped with healthier ideas concerning the 'germ-theory;'

and the adverse opinion still expressed upon it by many, which may be safely called the opinion of ignorance, will soon be heard no more.

Another class needs some knowledge of bacterial life. This includes all, — the father, the mother, the teacher, the citizen. Whoever has charge of human life should know something of the nature of infection with its manifold ways, of the necessity of disinfection and the means within reach. Education in such subjects is the only means of strengthening our present lax and indifferent spirit with reference to the public health. For this third and largest class a brief course of lectures, with demonstrations that will impress firmly the reality of the vital force inherent in bacteria, would be amply sufficient. What is needed is a certain attitude, an intelligent receptivity of the younger generations which will be favorable to all proper measures for the protection of public and private health, and which will promote in every way the study of the laws that underlie it.

The teaching of hygiene is taking root rapidly and firmly in the continental universities, and bacteriology is intrusted to such chairs. Our own higher institutions are beginning to realize the need of such instruction. As yet we have not gotten far beyond muscle, but that is a very good beginning. Bacteriology, though linked to hygiene as a branch of study, should, for the time being, find its place without difficulty in the biological laboratory.

THEOBALD SMITH.

Washington, D.C., Jan. 23.

Queries.

25. TREE TEMPERATURES. — In speaking with two farmers, each of more than ordinary intelligence, one last winter and another this, on the subject of temperature, they asserted that a thermometer hung against the trunk of a living tree of any size would not register as low as if suspended (as one made the observation) from a wire clothes-line, and the other from a pine post. Is this a fact?

D. LICHTY.

Rockford, Ill., Jan. 23.

26. THE EARTH'S ROTATION AS AFFECTING RAILWAY-TRAINS. — In Maury's 'Physical Geography of the Sea,' edition 1855, p. 39, paragraph 43 reads as follows: "Take for illustration a railroad that runs north and south. It is well known to engineers that when the cars are running north on such a road, their tendency is to run off the east side; but when the train is running south, their tendency is to run off on the west side of the track, i.e., always on the right-hand side. Whether the road be one mile or one hundred miles in length, the effect of diurnal rotation is the same; and the tendency to run off as you cross a given parallel at a stated rate of speed is the same, whether the road be long or short, the tendency to fly the track being in proportion to the speed of the trains, and not at all in proportion to the length of the road." Now, this article is quoted by many scientific authorities. It goes the yearly round of papers and periodicals. Is it true? To prove or disprove it, I have sent out a circular letter, to get from those familiar with railroads the facts on the subject. If it is true on a single-track road running north and south, with the same number of trains passing each way, the rails, and flanges of cars, not turned, would be equally worn. On double-track, the east rail north-bound, and west rail south-bound, would be most worn. Cars that were not turned would have their wheels and flanges equally worn; but locomotives, if "the tendency is always to the right," would have their right-hand flanges most worn. To facilitate the inquiry, I ask a list of questions. The questions are not asked for any personal advantage, but as of general scientific interest. 1. Do cars, when running north, have a tendency toward the east? 2. Do cars, when running south, have a tendency toward the west? 3. Have any instances come under your observation that indicate, by any wear of rails, of journals, of boxes, of flanges, or any part of a railway equipment, that "a train going north has a tendency to run off on the east side, but when the train is going south the tendency is to run off on the west side of the track"? 4. General remarks, with detailed description, — evidence *pro* or *con* from engines or rails.

JOHN C. GOODRIDGE, JR.

New York, Jan. 28.

Answers.

21. GLOBULAR LIGHTNING. — Governor Talmadge of Wisconsin lived in a two-story log-house on a level prairie near Fond du Lac, a short distance from a ridge of limestone that rose abruptly from the prairie. The upper story of the house had two rooms, with windows and doors forming a straight line through the house, and also an entry or hall between the rooms. One afternoon, when the windows and doors were open to allow a draught of air through the rooms, a ball, apparently a foot in diameter, floated slowly in one window, past Miss Talmadge, through the hall, and probably out of the other window, as the servant-girl ran screaming from that room. About the same time a barn near the house was struck and consumed. I could learn nothing further that was definite from those who saw the ball, when I reached the house.

T. MCDONOUGH.

Montclair, N.J., Jan. 24.

22. WASP-STINGS. — I have read with interest the items recently published in *Science* on this topic. Forty years ago, when a lad at school in Killingly, Conn. (in that part of the town at present known as Putnam Heights), I learned from schoolmates that any wasps could be handled without danger if one held his breath. I saw the experiment successfully made by many of my fellows, and ventured to make it myself with like results. Since that time scarcely a year has passed without my repeating the trial on wasps that have come in my way. I have never been stung except when I have forgotten myself, and allowed myself to inspire or expire the breath. Sometimes, after throwing the wasp violently away, I have been stung, because it had clung to my finger, and, not observing it, I had breathed. Ordinarily I notice after an experiment a slight feeling of numbness on the part of my hand where the wasp has attempted to sting me. I am accustomed to judge by this feeling whether the wasp was one of the stinging kind. As to the cause, I do not know of any. But many scientific persons have unscientifically refused to believe my statements, or to test them by experiment, because I could not answer their question, 'How do you account for it?' Whether the forced suspension of breathing paralyzes the nerves near the surface of the skin, — whether it stops the capillary circulation near the surface, — or whether its effect is something altogether different, I do not know. Nor do I see exactly how a paralysis of the superficial nerves, or an influence on the surface circulation, would prevent the poison from giving pain after commencing respiration again, provided that the wasp has succeeded in piercing the outer layer of the skin: for if the poison is exuded from the stinger, as I have sometimes seen it, it would act effectively upon removal of the paralysis when breathing is resumed. But my experience seems to lead to the conclusion that the poison does not penetrate at all during the suspension of the breath, but is left on the surface of the skin, and produces only the effect of a faint numbness after its effects begin to be felt through the outer coating. I do not take up this subject as one who has conducted any careful scientific experiments on it. My account of the matter may, however, help, like former articles in *Science*, in interesting experts in physiology to make genuine scientific experiments. One may hope that something important will be discovered in regard to the effect of forced suspension of the breath upon the nerves of feeling, the capillary circulation, or the resistance of the skin to penetration.

W. T. HARRIS.

Concord, Mass., Jan. 29.

23. DROPS OF WATER. — In answer to E. J. Pond's query in *Science* for Jan. 20, it seems to me that the phenomenon is explainable in the same way as the related phenomenon of drops of water on a hot stove; viz., rapid evaporation causes a layer of vapor to surround the drop, and this, by its repulsive expansion, keeps the globule of liquid from touching the hot metal in the one case, and the surface of the water in the other. The small drops that fall from the oar-blade will float a short time before calescing, even when no wind is blowing; the fall through the air apparently setting up evaporation enough to bear up the tiny globule. I have seen them at night, when the air was perfectly still, gleaming like seed-pearls in the moonlight. When the wind is strong, much larger drops will be supported because of the rapid evaporation.

C. M. WIRICK.

Metropolis, Ill., Jan. 24.